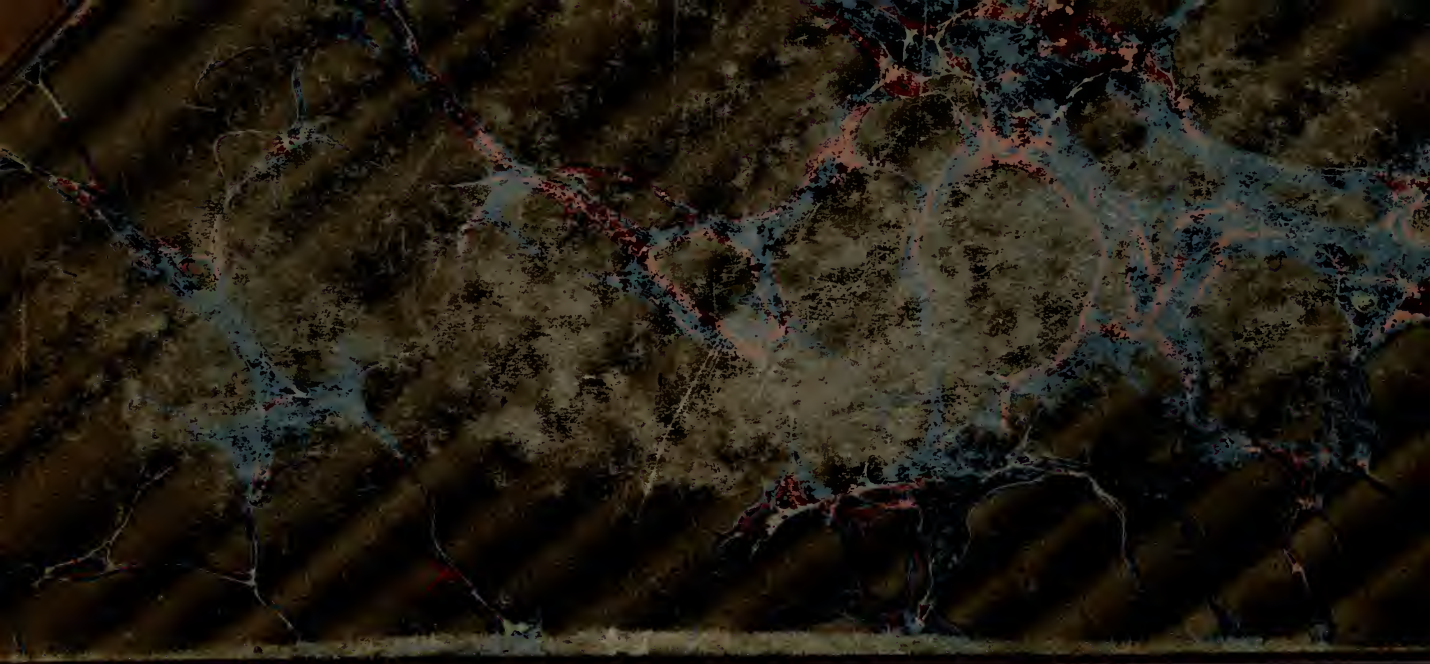

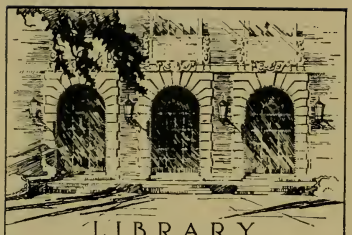

LIBRARY
OF THE
UNIVERSITY
OF ILLINOIS

THE

USELESSNESS OF VIVISECTION UPON ANIMALS

AS A METHOD OF SCIENTIFIC RESEARCH.

BY

LAWSON TAIT, F.R.C.S., ETC.

READ BEFORE THE BIRMINGHAM PHILOSOPHICAL SOCIETY
APRIL 20, 1882, AND REPRINTED, BY PERMISSION
FROM THE SOCIETY'S TRANSACTIONS.

BIRMINGHAM:
THE HERALD PRESS, UNION STREET.
1882.

[*REPRINTED FROM THE PROCEEDINGS OF THE BIRMINGHAM PHILOSOPHICAL SOCIETY.*
VOL. II., PAGE 121, ETC.]

VII.—*On the Uselessness of Vivisection upon Animals as a Method of Scientific Research.*

BY LAWSON TAIT, F.R.C.S., &c.

[Read before the Society, April 20th, 1882.]

I NEED make no apology for adopting the same title for this paper as that of Mrs. Kingsford's article in the *Nineteenth Century* for January last, because I had advanced this plea against Vivisection some time previous to the appearance of her contribution, and the more I know of the question, the more fully convinced do I become of the verdict which will ultimately be passed upon it, both by the public and by the medical profession.

I need not go into the general history of Vivisection, for it hardly bears upon the question to which I desire to limit myself; but I think it advisable to formulate a few preliminary conclusions before I come to my immediate subject, in order that I may clear the way for discussion, and show at once the grounds upon which I stand, for I find myself in a position adverse to the view adopted by the great majority of my professional brethren.

I dismiss at once the employment of experiments on living animals for the purpose of mere instruction as absolutely unnecessary, and to be put an end to by legislation without any kind of reserve whatever. In my own education I went through the most complete course of instruction in the University of Edinburgh without ever witnessing a single experiment on a living animal. It has been my duty as a teacher to keep

myself closely conversant with the progress of physiology until within the last four years, and up to that date I remained perfectly ignorant of any necessity for vivisection as a means of instructing pupils, and I can find no reason whatever for its introduction into English schools, save a desire for imitating what has been witnessed on the Continent by some of our most recent additions to physiological teaching. In Trinity College, Dublin, the practice has been wholly prevented, and on a recent visit to that institution I could not find, after much careful inquiry, the slightest reason to believe that any detriment was being inflicted upon the teaching or upon those taught.

The position of vivisection as a method of scientific research stands alone amongst the infinite variety of roads for the discovery of Nature's secrets as being open to strong *prima facie* objection. No one can urge the slightest ground of objection against the astronomer, the chemist, the electrician, or the geologist in their ways of working; and the great commendation of all other workers is the comparative certainty of their results. But for the physiologist, working upon a living animal, there are the two strong objections: that he is violating a strong and widespread public sentiment, and that he tabulates results of the most uncertain and often quite contradictory kind.

I do not propose to deal with the sentimental side of the question at all, though no one can doubt it is a very strong element in the case as maintained by public opinion, but I must point out that there are four avenues of thought by which this aspect of the case is almost unconsciously traversed, and which are to be separated from it only by arbitrary divisions.

The first is the avenue of pure abstract morality, by which it is argued that we have no right to inflict sufferings on others that we ourselves may benefit, an avenue which is worthy of the highest respect, because its opening up is only a matter of yesterday in the evolution of the moral life of individuals, and as far as national morality is concerned it can hardly be said to have been ever seriously considered until about a year ago.

The Rare Book & Manuscript Library UIUC

The second may be called a political avenue, and is also one of importance, though that importance is not visible at first sight, and may even be altogether denied by some of a particular shade of political conviction. But to those of us who regard the Game Laws as a prolific method of manufacturing criminals, of wasting public money, of preventing the development of agricultural industry, and hindering the development of the peasant from his present serfdom to his possible and perfect citizenship, this avenue assumes a mighty importance when we discover that the lay support of vivisection is derived mainly from those who maintain costly pheasant preserves in order to become amateur poultry butchers, and who maim pigeons at Hurlingham under the idea that it is amusement.

Any one, therefore, who objects to the Game Laws from political conviction, will put vivisection upon its trial, and he must hear a good case before he consents to an acquittal.

The third avenue is the religious one, and it is a road many are travelling, upon very different errands, and with very different convictions. I must content myself with pointing out that the doctrine of evolution has affected religion as it has everything else, if indeed it is not establishing an altogether new form of faith, which is making an unrecognised, certainly an unmeasured, progress amongst us. Admitting that the so-called lower animals are part of ourselves, in being of one scheme and differing from us only in degree, no matter how they be considered, is to admit they have equal rights. These rights are in no case to be hastily and unfairly set aside, but should be all the more tenderly dealt with in that civilisation and inventions are every day making it more and more difficult for the animals to assert their independence, or as it were to vote upon the question.

There remains, therefore, the fourth avenue, which simply amounts to the inquiry, Has this method of scientific research—vivisection—contributed so much to the relief of suffering or to the advance of human knowledge as to justify its continuance in spite of the manifest objections to it? My own answer I shall

try to give in the following pages, merely premising that an answer to justify vivisection must be clear and decisive, must be free from doubt of any kind, and above all, it must not assume the protection of a "privileged mystery." This is a question, I maintain, which can be discussed by an educated layman just as well, perhaps better, than by a physician or a surgeon or a professional physiologist. It is a question chiefly of historical criticism, and we must have a conclusive answer concerning each advance which is quoted as an instance, how much of it has been due to vivisectional experiment and how much to other sources, and this amount must be clearly and accurately ascertained. It will not do, as has been the case in many of the arguments, to draw such a picture as that of an amputation in the seventeenth century and one performed last year, and say that the change is due to vivisection. We might just as well point to the prisons of the Inquisition and then to one of our present convict establishments and claim all the credit of the change for the fact that our judges wear wigs. The real questions are: What advances in detail are due to vivisection? Could these advances have been made without vivisection? If vivisection *was* necessary for elementary and primitive research is it any longer necessary, seeing that we have such splendid and rapidly-developing methods in hundreds of other directions? Have we made complete and exhaustive use of all other available methods, not open to objection? And finally, are the advances based upon vivisection of animals capable of being adapted conclusively for mankind, for whose benefit they are professedly made?

It must be perfectly clear that to answer all these questions, specific instances must be given, and that they must be analysed historically with great care. This has already been done in many instances, and I am bound to say in every case known to me, to the utter disestablishment of the claims of vivisection.

Take the case of the alleged discovery of the circulation of the blood by Harvey, and it can be clearly shown that quite as

much as Harvey knew was known before his time, and that it is only our insular pride which has claimed for him the merit of the discovery. That he made any solid contribution to the facts of the case by vivisection is conclusively disproved, and this was practically admitted before the Commission by such good authorities as Dr. Acland and Dr. Lauder Brunton. The circulation was not proved till Malpighi used the microscope, and though in that observation he used a vivisectional experiment his proceeding was wholly unnecessary, for he could have better and more easily have used the web of the frog's foot than its lung. It is, moreover, perfectly clear, that were it incumbent on any one to prove the circulation of the blood now as a new theme, it could not be done by any vivisectional process, but could at once be satisfactorily established by a dead body and an injecting syringe. In fact, I think I might almost say that the systemic circulation remained incompletely proved until the examination of injected tissues by the microscope had been made.

But supposing we grant, for the sake of argument, that such an important discovery had been made by vivisection and by it alone, there still remains the all-important question, is it necessary to use such mediæval methods for modern research? No one can doubt that the rude methods employed in Charles II.'s reign for obtaining evidence—the rack, the boot, the thumb-screw, and the burning match—were occasionally the means of accomplishing the ends of justice, but need we go back to them now? The very necessity for ending them brought into use fresh and far less fallible methods, and I am inclined to make the claim for physiology, pathology, and the practice of medicine and surgery that the very retention of this cruel method of research is hindering real progress, that if it were utterly stopped, the result would certainly be the search for, and the finding of, far better and more certain means of discovery. To urge its continuance on the ground that it was useful in the seventeenth century is just as reasonable as to ask the astronomer to go back to the cumbrous tackle by which Huyghens first worked his lenses.

If the method of obtaining evidence by torture was occasionally successful, there can be little doubt that as a rule it failed and led the inquirers astray. So I say it has been with vivisection as a method of research, it has constantly led those who have employed it into altogether erroneous conclusions, and the records teem with instances in which not only have animals been fruitlessly sacrificed, but human lives have been added to the list of victims by reason of its false light.

Those who have recently advocated vivisection seem to have forgotten or to have ignored this most fatal objection, and as a rule they have indulged in a line of argument which is little more than assertion. For the purpose of this paper I have gone carefully over a large mass of literature upon the subject, and find that the bulk of it is altogether beyond criticism, because it does not deal with fact. Thus in a recent address on the subject by Professor Humphrey, of Cambridge, there is a long list of advances in medicine and surgery, every one ot which is attributed to vivisection solely because some experiments were mixed up in the history of each instance ; but not an effort was made to show that the advances were due to vivisection. The proper method for the discussion of this subject is to take up a number of special instances and to subject them to careful criticism, chiefly by historical evidence, and as soon as the advocates of vivisection do this successfully, I am prepared to grant their case. But hitherto they have failed.

Serial literature during the last few months has been singularly fertile in articles on the question of vivisection, and one commanding attention as an editorial is to be found in *Nature* of March 9th.

There the *a priori* argument for vivisection is put in the familiar illustration that " it would be more reasonable to hope to make out the machinery of a watch by looking at it, than to hope to understand the mechanism of a living animal by mere contemplation." Unfortunately there is a fault in the analogy, and it may be far more truly put in the converse, that it would be wholly impossible to repair the damaged movements of a

watch by experimenting with an upright pendulum clock. There is a perfectly parallel dissimilarity between the functions and the diseases of animals and those of man.

In the same article is a quotation from the article of Sir William Gull, to the effect that the experiments of Bernard, in baking living dogs to death in an oven have opened the way to our understanding the pathology of fever. In zymotic diseases the elevated temperature is not a cause of the disease but its consequence, and the answer to the argument is that not a single contribution of any kind has yet been made to the cure of scarlet fever. Its course cannot be shortened by one hour Medicine is powerless for the cure of zymotics, whilst hygiene is all-powerful in their prevention, and the medicine of the future lies wholly in this direction. Drugs are impotent, but sanitary laws can and will banish all these diseases, when they are completely understood and fulfilled.

The article continues that "between 1864 and 1867 seven new drugs were added to the Pharmacopœa, of which at least the two most useful, carbolic acid and physostigma, are due to vivisection." Upon the question of new drugs I can speak only with great reserve, for such a wholesome scepticism concerning drugs has been introduced by the medical schism of homœopathy, that I look upon all new drugs with great suspicion. Sir William Gull himself says he has not much belief in drugs. I fear most new drugs do more harm than good; some of them, such as chloral, most certainly have done so. I cannot learn that physostigma is of any practical service, and I have shown in my published writings that carbolic acid has done far more harm than good. Perhaps it would have been better if we had never heard of it. The question of the investigation of the actions of drugs by experiments on animals I have to confess is a very difficult one, because after we have found out what they do in one animal we find that in another the results are wholly different, and the process of investigation has to be repeated in man. Not only so, but in human individuals the actions of drugs in very many cases vary so much, that each fresh patient

may form really a new research. Pharmacy forms, therefore, at least, a very shaky argument for vivisection.

Finally, the Editor of *Nature* deals with the argument of proportion, which is stated to the effect that the proportion of pain inflicted by vivisection bears but small ratio to the pain relieved by the discoveries effected in that way. But if this question be examined historically, as it must be for the sake of justness, it will be found that the argument is all the other way. To take the case of Ferrier's experiments, if the history of the point be examined, even from the period of Saucerotte till now, the number of experiments recorded is perfectly awful, and we can easily imagine that many more were performed and not put on record. Concerning the arteries this is still more true ; and it is, to say the least of it, very doubtful if any permanent good has been done by them. What we do really know about both of these matters with certainty has been derived from the post-mortem examinations of our failures in human subjects, and not from vivisection experiments.

In a work published within the last few weeks by a distinguished member of this Society, Dr. George Gore, entitled "The Scientific Basis of National Progress," and at p. 80, will be found the following sentence: "The Antivivisection movement is but one of the phases of the ever-existing conflict between the advancing and retarding sections of mankind."

I do not know whether I belong to the antivivisection movement or not, but I certainly cannot rank myself with those who attribute to vivisection the merit which distinctly belongs to other causes. So far I am an antivivisectionist most thoroughly.

Similarly I do not know whether or not I am to be regarded as belonging to the "retarding section of mankind." If I am so classed I fear I shall be in company as strange to me as I shall be objectionable to it. But my relief is great as I read further in Dr. Gore's book and see upon what grounds he has built his conclusion. I have never heard that Dr. Gore has conducted any vivisection research himself, and therefore I

assumed that he took his argument from some other source. He was kind enough to give me his reference for the following statement, which he makes at page 81: "Ferrier's comparatively recent vivisection experiments have already enabled medical men to treat more successfully those formidable diseases, epilepsy and abscess of the brain." His authority is an anonymous article in the *British Medical Journal* of November 19th, 1881, in which a series of cases is given in support of this extraordinary statement. The purport of it is that the experiments of Ferrier have led to greater certainty in applying the trephine for the removal of depressed fractures, etc., which had produced serious symptoms, or for the relief of matter in cerebral abscesses.

I do not propose now to go into this very wide and difficult question, because I shall have a fuller opportunity on another occasion. I shall only say that Ferrier's first experiments were published in 1873, and that previous to that time a large number of cases are on record where the seat of injury was ascertained with perfect accuracy by simpler and less misleading methods—in one case by myself in 1868. The *a priori* difficulties in the application of Ferrier's conclusions are enormous and, as it seems to me, insuperable; and, after a most careful historical consideration of the illustration quoted by Dr. Gore, my verdict is most decidedly that of *not proven.*

The application of the trephine for the treatment of epilepsy is of course absolutely limited to cases where the disease is the result of injury to the skull. No one has ever dreamed of applying it to other cases. I find that the first operation of this kind was performed in 1705, by Guillaume Mauquest de la Motte with partial success, and it was repeated with complete success by Mr. Birch of St. Thomas's Hospital, 1804. Between 1804 and 1865 there are 50 cases on record (collected by Dr. James Russell, *British Medical Journal*, 1865), and of these 44 recovered, the results being satisfactory in 39 of them. This paper of Dr. Russell's was published years before any of Ferrier's experiments were undertaken, and the results of trephining for

epilepsy published since are not so good as those published by Dr. Russell. The most recent contribution to the subject is a paper by Mr. J. F. West, who asks the question "Are our indications in any given case, either of paralysis or epilepsy, sufficiently precise and well-marked to warrant us in recommending the use of the trephine at a particular point of the skull? and he answers it thus: "It will be a long time before it is definitely settled, but such cases as those alluded to give encouragement." This answer of a practical surgeon is very different from that of Dr. Gore.

Even if the conclusions which are attributed to Dr. Ferrier's researches were to be regarded as indisputable, my answer would be that they might have been arrived at, and certainly would soon be enormously extended, if our clinical research were conducted upon reasonable and scientific principles. The chief reason of the slow advance of the arts of medicine and surgery is the reckless waste of the material so plentifully supplied by disease, and the first remedy will consist in the sub-division of the labour, a remedy against which, unfortunately, the medical profession protests most vigorously.

It is of course perfectly impossible to deal with all of the illustrations in favour of vivisection which have recently been advanced in the limits of an ordinary paper, and I prefer to take those which deal with points of practical utility, rather than with such as have as yet only a possibility of being useful in the future. I shall deal, therefore, at present chiefly with the illustrations which have been gathered from the field of practical medicine and surgery, for in them, of course, the public see the strongest arguments. If it is publicly announced, as has been done of late very widely, that human diseases have been cured and human suffering lessened by experiments on the lower animals, the public must therein see a strong argument for vivisection. But such announcements are open to the test of historical examination, and to this I propose to subject the most important of them. I am equally open to discuss in the same way those points of less apparent usefulness, the matters of mere

physiological discovery, on some future occasion, if it should arise; but, as with these, the only defence can be, that some day they may prove of service, it is clearly best to deal first with those for which an actual and not merely a potential utility is claimed.

Those of my professional brethren who take the other side may probably complain that I have selected a lay audience for the discussion; but the answer is, that by the circulation of pamphlets, and by communicated paragraphs in newspapers, they have already taken the initiative, and I am but meeting them on their own ground.

I am quite well aware that I am one of a small minority of my profession in my view that vivisection is useless as a method of research, but the answer I am disposed to offer on this point is, that not one in a hundred of my professional brethren have ever seriously examined the question. Ninety-nine take for granted the statements of the hundredth, and he, in turn, has not gone into the matter upon that side from which alone a safe answer can be given—that of historical criticism.

The dispute, as I have already said, is not to be settled by mere statement of opinion, one way or the other; nor is it a question of authority. On the argument of authority a very singular answer has been given by the supporters of vivisection in the case of the late Sir William Fergusson, who stated in his evidence before the Royal Commission that in his opinion nothing had been gained for surgery by experiments on the lower animals—an opinion which I entirely endorse. During his lifetime, Sir William Fergusson had heaped upon him all the distinctions which his Queen, his country, and his profession had it in their power to bestow. He was the titular head of his profession, its most successful operator, one of its greatest anatomists, its most widely employed practitioner, its most successful teacher, the author of its principal text-book on surgery—but now, when he is dead, we are told he was not a scientific surgeon, because he did not believe in vivisection. Nobody said this in his lifetime, and so late as 1873 he was

elected President of the British Medical Association, over all the profoundly scientific surgeons of the Metropolis. I share Sir William's opinions concerning vivisection, and I am quite content to rank with him on that account as an unscientific surgeon.

A pamphlet has recently been published in this town on "The Influence of Vivisection on Human Surgery," by Mr. Sampson Gamgee, in which the proposition is set forth that without experiments on living animals "scientific surgery could not have been founded, and its present humane and safe practice would have been impossible." Mr. Gamgee supports this proposition by a series of instances which we may presume are the best and strongest he could find. These I tabulate as follows, and I shall discuss them historically in this order.

I. Treatment of injuries of the head, and the theory of Contre-coup.
II. Amputation of the Hip-joint.
III. Paracentesis Thoracis.
IV. Sub-cutaneous Tenotomy.
V. Treatment of Aneurism, Ligature, and Torsion of Arteries.
VI. Transfusion.
VII. Abdominal Surgery.
VIII. Function of periosteum.
IX. The Ecraseur.
X. Detection of Poison.

Mr. Gamgee tells us that the Académie de Chirurgie gave out the subject of contre-coup, and its influence in injuries of the head as the subject for a prize competition, and that the prize was obtained in 1778 by M. Saucerotte, whose essay was based "on literary research, clinical observations, and twenty-one experiments on living dogs."* He omits, however, to make

*Memoire sur les Contre-coups dans les Lésions de la Tête, par M. Saucerotte (Couronné en 1768), Mem. Acad. de Chirurgie, tom. x., 327, *et seq.*

any estimate of the value of the experiments on the dogs, which seems to me to be absolutely nothing; and he quite forgets to mention that the theory of contre-coup had been completely established for nearly two centuries before, and had been particularly the subject of Paul Ammannus of Leipsic, who wrote a well-known work, "De resonitu seu contra fissura cranii," in 1674, in which trepanning is recommended at the point of contre-coup, as had been practised by Paul Barbette, of Amsterdam, thirteen years before that. The theory of contre-coup, and the fatal practices arising from it, are happily now buried in oblivion, in spite of Saucerotte's vivisection, and would never again have been alluded to, but for Mr. Gamgee's unfortunate resurrection of them.

The modern verdict concerning fractures of the skull is given tersely in Mr. Flint South's words, "the less done as regards meddling with them the better," and "a knowledge of counter fractures is quite uncertain." In fact nothing could be more unfortunate than the selection of M. Saucerotte's experiments as an illustration of the value of vivisection, for they were performed for a purpose which was long ago recognised as futile, and in support of a practice universally condemned.

M. Saucerotte says—"Pour établir le diagnostic des lésions des différentes parties du viscère, j'ai cru devoir prendre la voie de l'expérience et de l'observation. Ce ne sont point ici des conséquences hasardées, ce sont les résultats de faits pénible, que formeront, á ce que j'espère un foyer lumineux, dont les rayons répondront le plus grand jour sur la pratique." He anticipated many of Ferrier's experiments by more than a hundred years, and when he trephined the skulls of dogs and injured their brains on the right side, he found that they became somewhat feeble on their left sides, and *vice versa*, a fact that had been established by pathology long before. His idea of imitating the injury of contre-coup, was to pass a knife right through the substance of the brain, till it impinged on the inner surface of the skull opposite the trephine hole, a most absurd experiment, as the contre-coup injures at the opposite

surface only, and not necessarily at all the intervening brain substance.

Reading his experiments, they seem so like Ferrier's that I fancy if Dr. Ferrier had known of the existence of this essay he would have found little need to repeat its work.

Many of the conclusions of Saucerotte's experiments are eminently absurd, and, save that of the decussation of the fibres, which was known before, I can find few that have been since accepted, and those that have been he candidly avows were previously observed in cases of disease. Finally, the conclusions concerning treatment of injuries of the head which he draws from his experiments are not such as would be listened to in modern surgery, and it is certain that if they were ever acted upon they must have had results almost uniformly disastrous.

The fact is, that the whole run of vivisectional experiments on the brains of animals, now extending over hundreds of years, have given no sort of assistance to the elucidation of the physiology of that wonderful organ, so contradictory have been the results. On this subject Dr. W. B. Carpenter, who curiously enough has recently appeared as an ardent supporter of vivisection, says, in the seventh edition of his standard work on the "Principles of Human Physiology," p. 645, "The results of partial mutilations are usually in the first instance a general disturbance of the cerebral functions; which subsequently, however, more or less quickly subsides, leaving but little apparent affection of the animal functions, except muscular weakness. The whole of one hemisphere has been removed in this way, without any evident consequence, save a temporary feebleness of the limbs on the opposite side of the body, and what was supposed to be a deficiency of sight through the opposite eye. * * * So far as any inferences can be safely drawn from them these experiments fully bear out the conclusion that the cerebrum is the organ of Intelligence," a conclusion which surely has never been doubted, since it was first the object of the then savage club to destroy the intelligence of a foe by cracking his skull. Continuing his researches on such experi-

ments as those of Saucerotte and Ferrier, Dr. Carpenter tersely sums up the *prima facie* objections to them, objections which seem to him, as they seem to me, to be fatal to their utility: "It is obvious that much of the disturbance of the sensorial powers which is occasioned by this operation is fairly attributable to the laying open of the cranial cavity, to the disturbance of the normal vascular pressure, and to the injury necessarily done to the parts which are left by their severance from the cerebellum." Dr. Marshall Hall also pointed out long ago that injury to the dura-mater is an important factor in the results obtained.

II.—Amputation of the Hip Joint.

At page 8 of his pamphlet, Mr. Gamgee makes the astonishing statement that this operation was only attempted after it was proved safe by vivisection. The authority he has been kind enough to give me for this is a brief sentence in the preface to the ninth volume of the "Memoires de l'Académie de Chirurgie," written by the Secretary General and published in 1778.

But the first hint we get of amputation of the hip-joint is from a German surgeon named Vohler, who was in practice about 1690. It is doubtful if he ever performed it on a living patient, but it is on record that he tried on the dead body. But it was performed by M. la Croix, of Orleans, in 1748, not only on one limb, but on both limbs of the same patient, the first operation being successful, and the second almost so. This was nearly thirty years before the publication of the vivisection of dogs; and there are many other cases of success previous to Mr. Gamgee's alleged origin of the operation, one being by the celebrated Ker of Northampton, in 1773; and as Mr. Gamgee has published a large book on amputation of the hip-joint, it is surprising that he did not know something more about the history of the operation.

III.—Paracentesis Thoracis.

Mr. Gamgee makes another most unfortunate selection in the case of William Hewson, who based a theoretical operation for pneumothorax upon experiments on living dogs and rabbits so long ago as 1769. He made a wound in the side of the chest and admitted air into the pleura, where no air ought to be, and then he operated to get it out again. When such a condition is brought about in man, and no vital organ seriously injured, the patient gets perfectly well without any operation. I cannot learn that Hewson's operation for the removal of air has ever been performed on man. When pneumothorax occurs from disease it is generally associated with conditions necessarily fatal, for which no operation is advisable. On this point the greatest authority, Dr. Bowditch of New York, says, "I have operated once in pneumo-hydrothorax, with temporary relief and comparative ease for several days. Many theoretical objections may be urged against the operation in such a case; but as the operation can do no harm and may give much relief, I shall operate again in such a case." The proceeding is therefore doubtful, the conditions are extremely rare, pure pneumothorax, such as Hewson invented his proceedings for, never needs it, and therefore his experiments on living dogs and rabbits were useless.

Finally, tapping for the removal of fluid in the chest was practised long before Hewson's time, and therefore his research was needless. Hewson really based his proposal on this well-known practice, but in this he was anticipated in the most favourable cases—those of wounds—for Anel, of Amsterdam, published quite the same proposal in 1707, and it has been uniformly condemned by every writer on military surgery since, because the removal of the air merely induces bleeding.* Anel devised a syringe for the purpose, which has been revived as the modern aspirator.† Had Mr. Gamgee known anything of Dominic Anel he would never have mentioned William Hewson.

---

* Flint South's edition of Chelms, vol. i, p. 452.

† L'Art de Sucer les Plaies sans le servir de la bouche d'un homme. Amsterdam, 1707.

IV.—Subcutaneous Tenotomy.

I have traced the history of the surgery of tendons, and I cannot see the slightest reason to attribute any of the advances in this department to the alleged vivisections of John Hunter. I cannot find any record of these experiments, beyond the allusions to them by Drewry Ottley, and Palmer in his life of Hunter.

The same accident which happened to Hunter in 1767 happened to the first Monro in 1726, and from the latter instance a very marked advance in surgical practice was at once made, and a contrivance invented by Monro himself, for his own case is still in use and goes by his name. No such advance was made from Hunter's accident or from his vivisections. In their histories of the progress of orthopœdic surgery Little and Adams make no such claim for Hunter. Adams points out clearly, and with justice, that Hunter established the principles on which subcutaneous surgery is now conducted; but these he established from clinical observations, not from experiments upon animals. And in his lecture on "Ruptural Tendons" (vol. i., p. 436) Hunter says not one word about his vivisections, or any conclusions he derived from them as to the method of repair of tendons. If he ever made any such experiments he must have placed very little value upon them.

If we trace the development of tenotomy we find that Hunter's experiments had no influence upon it at all. They were performed, it is said, in 1767. But the first tenotomy was not performed till 1784, by Lorenz, at Frankfort, and then the conditions were absolutely in defiance of the principles of subcutaneous surgery. It was done by an open wound, and this practice was continued with hardly any modification till far on in this century. In fact, as Adams points out, it is from 1831 that the commencement of scientific tenotomy dates, at the hands of Stromeyer. If this is so, and Adams makes his case out most conclusively (Club-Foot, 1873), how utterly use-

less Hunter's experiments on dogs must have been, to lie forgotten and unnoticed till unearthed in Mr. Gamgee's pamphlet of 1882, one hundred and fifteen years after they were performed; or how singularly careless, and inattentive to the teachings of vivisection the medical profession must be, that they should allow this immense discovery to lie neglected from 1767 till 1831.

To bring forward so rash an illustration as this for the value of vivisection is to cast a terrible slur at the profession of surgery, a slur which I do not think at all deserved if the true history of such advances is carefully investigated, and the moving causes of them properly credited.

V.—Treatment of Aneurism, Ligature and Torsion of Arteries.

Mr. Gamgee alludes to the oft-quoted story of the Hunterian operation for aneurism as a proof of the aid vivisection has given to surgery. This illustration has been so completely and so often destroyed, that it is absolutely unnecessary to allude to it further than to explain that Hunter modified Anel's operation merely because he found the artery near to the seat of disease would not hold the ligature, and the patients bled to death. As the arteries of animals never suffer from the disease in question experiments upon them could not have helped Hunter in any way whatever. Sir James Paget, who has lately appeared as an ardent advocate for vivisection, and, therefore, may be appealed to by me as a witness not biassed to my view, has recorded his opinion in the Hunterian oration given at the College of Surgeons in 1877, that Hunter's improvement in the treatment of aneurism "was not the result of any laborious physiological induction; it was mainly derived from facts very cautiously observed in the wards and deadhouse." In this opinion Sir James Paget is undoubtedly correct.

Concerning the tying and torsion of arteries I am in a position to speak with some authority, because I have myself

performed experiments on living animals, and have found how futile they are, and how uncertain and untrustworthy are their results. Mr. Gamgee tells us that some local worthies, who distinguished themselves by early performances of serious operations, practiced their 'prentice hands on living animals. This is not scientific experimentation, but culpable and wholly unnecessary cruelty. It is on the dissecting table that a surgeon prepares his hand for his work, and not on the bodies of living animals. I have never known nor heard of such an instance before, and I trust there are no more to be quoted. Any surgeon who did this now would, I am sure, receive a universal condemnation from his professional brethren.

Mr. Gamgee quotes Jones's experiments on the arteries of animals as an instance of a valuable contribution to surgical progress by experiments on animals, and I do not think any more complete illustration could be quoted in support of the uselessness of vivisection as a method of scientific research than that of the history of the physiological and pathological processes to be observed in arteries. If we consider the question from what some would call the purely scientific side, that is apart altogether from any practical bearings it may have for the relief of human sufferings and the cure of human disease, it consists merely of a mass of observations in which each observer contradicts some other. Upon this subject I wrote as follows so long ago as 1865 :—

"John Hunter warned surgeons to avoid injuring any of the coats of an artery, and to this effect advised that the ligature should not be drawn so tight as to cut them ; while many of his contemporaries and successors dreaded any injuries so much that they used all sorts of clumsy contrivances to avoid it—such as pads of lint and bits of cork inserted between the arteries and ligature. Again, Travers, in his experiments on ligatures of arteries, demonstrated that Jones was quite wrong when he insisted that it was necessary to divide the inner coats ; and Mr. Dalrymple, of Norwich, proved by his experiments that while simple and continued contact of the parietes of a vessel, without the slightest wound of any of the coats, was sufficient to produce permanent adhesion and obliteration, yet that division of the internal and middle coats without continued coaptation

invariably failed to produce adhesion. Hodgson says that he cannot substantiate Jones's statement that division of the coats is essential, and strongly supports the opinion that coaptation of the walls, without rupture of any of the coats, will produce occlusion. The theories of Dr. Jones were strongly supported by Professor Thompson, his teacher, but were strongly opposed by Sir Phillip Crampton, who insisted that the division of the coats not only was unneccessary, but that it frequently defeats its own object."—(*Medical Times and Gazette, 1865.*)

I quote this at length to show that fifteen years ago I found authorities differing so much on this scientific question that I thought it advisable to institute a new series of vivisectional experiments to decide it. The experiments performed by myself only added to the confusion, though nobody saw that at the time. What we were working at was to get quit of the ligature altogether, and to secure arteries by a temporary compression of some kind without injuring the coats. Acupressure promised to accomplish this; but it failed, for reasons I need not enter into here. The desire to get quit of the ligature was due to the fact that after a vessel was tied one end of the ligature was cut off and the other left hanging out of the wound, where it remained for weeks, sometimes for months, and occasionally (as in Lord Nelson's case) for years.

The amazing thing is that with all the experiments made upon animals nobody ever thought of cutting both ends of the ligature quite short and closing the wound over it. As a matter of fact, from the time of Ambrose Parè to that of Simpson, an interval of over 300 years, we went bungling on with experiments on animals when the whole thing lay clear before us. It was the successful experiments of Baker Brown, and Thomas Keith upon women suffering from ovarian tumours which showed us that if we use pure silk, cut the ends of the ligature short, and close the wound carefully over them, success will be certain. Yet not content with this, we hear of fresh experiments on animals with carbolised catgut, chromicised catgut, kangaroo tendons and other novelties, which speedily die out when applied to human beings.

In the case of the arteries, therefore, experimentation on animals has proved to be "science, falsely so called." What we have done in this direction is entirely the result of clinical experience, and that only.

VI.—Transfusion.

This operation was not initiated, as asserted by Mr. Gamgee, in the second half of the seventeenth century by Dr. Lower, of Oxford, nor was it first proposed as a legitimate surgical operation at all. It was proposed, and in all probability was really practised, by the alchemists of the sixteenth century as an attempt to obtain for the wealthy aged a renewal of their lease of life, after the theory and legend of Faustus. Certain it is that allusions to it are frequent, though the first actual account of its performance is given by André Libavius, Professor of Medicine at Halle (Helmst. 1602), as having been performed by him in 1594, the blood of a young healthy man being transfused into a man aged and decrepit, but able and willing to pay for the supposed advantage. In the early part of the seventeenth century, it was a good deal discussed from this point of view, forgotten for a while, and then after the Restoration it was reconsidered, and a great deal written about in this country and on the Continent. An extremely interesting allusion to the experiments is to be found in the wonderful Diary of Samuel Pepys :—

"November 14th, 1666.—Dr. Croone told me, that at the Meeting at Gresham College to-night (which, it seems, they now have every Wednesday again), there was a pretty experiment of the blood of one dog let out (till he died) into the body of another on one side, while all his own run out on the other side. The first died upon the place, and the other is very well, and likely to do well. This did give occasion to many pretty wishes, as of the blood of a Quaker to be let into an Archbishop, and such like; but, as Dr. Croone says, may, if it takes, be of mighty use to man's health, for the amending of bad blood by borrowing from a better body.

16th.—This noon I met with Mr. Hooke, and he tells me the dog which was filled with another dog's blood at the College

the other day is very well, and like to be so as ever, and doubts not it's being found of great use to men, and so does Dr. Whistler, who dined with us at the Tavern."

The scheme of transfusion in all the experiments of the seventeenth century descriptions of which I have seen, was to take arterial blood from an animal and pass it into the veins of another, and that this was successful is not surprising. But this has never been attempted in modern times upon man. It certainly would not be justifiable; because, to interfere with a large artery—and a large artery would be required—in a man is always an extremely risky thing. Dr. Lower, who is Mr. Gamgee's authority, in 1667 injected or tried to inject arterial blood from a lamb into a man, but the operation was so badly done that I do not believe any blood really passed. If Pepys' idea could have been carried out, of transferring some of the peaceful blood from the arteries of a member of the Society of Friends, for the replacement of the turbulent and brutal spirit of Archbishop Laud, some good might have been done, much of the terrible history of that time need not have been written, and I might not have appeared here as a critic of such experiments. But no such or any other good result was obtained. A large army of experimenters rushed into the field, a fierce controversy took place; but before the eighteenth century dawned the whole thing was discredited and forgotten. Mr. Flint South gives a succinct history of the matter, and tells us that it was revived by the plan of mediate transfusion in the early part of the present century. The former experiments were fruitlessly repeated and others tried. The result is that the operation has a very insecure hold on professional opinion. I have seen it performed seven times without success in a single instance. I have twice been asked to do it, and have declined, and both patients are now alive and well. We hear a great deal of cases in which patients have survived after transfusion has been performed, but we hear little or nothing of its failures. Personally, I have no confidence in the proceeding.

VII.—Abdominal Surgery.

Mr. Gamgee alludes to a vivisection experiment made by John Shipton, and published in 1703, as having laid the foundation for the recent advances of abdominal surgery, which are attracting the admiration of the whole professional world, and the instances he quotes date so late as 1880. If Shipton's experiment has been so fertile, why has the crop been delayed for one hundred and seventy seven years?

But even here Mr. Gamgee is wrong in his history. The whole progress of abdominal surgery dates from the first successful case of ovariotomy performed by Robert Houston in 1701. Failing to see the lesson taught by this, and led astray by vivisection, no further success was achieved till 1809, by Ephraim McDowell, and it was not till 1867 that any substantial gain was made. Disregarding all the conclusions of experiment, Baker Brown showed us how to bring our mortality of ovariotomy down to 10 per cent.; and again, in 1876, Keith proved that it might be still further reduced. The methods of this reduction were such as only experience on human patients could indicate; experiments on animals could and did teach nothing, for operations have been performed on thousands of animals every year for centuries, and nothing whatever has been learnt from this wholesale vivisection.

As soon as Keith's results were established abdominal surgery advanced so rapidly that now, only six years after, there is not a single organ in the abdomen that has not had numerous operations performed upon it successfully. I have had, as is well known, some share in this advance, and I say without hesitation, that I have been led astray again and again by the published results of experiments on animals, and I have had to discard them entirely.

Speaking of some recent attempts which have been made to operate on cases of cancer of the stomach, Mr. Gamgee says: "Warranting, as such cases do, the placing of cancer of the stomach amongst diseases curable by the knife, do they not also

justify the vivisection of dogs by Shipton and Travers, who, by their experiments, laid the first scientific foundation of intra-abdominal surgery?" Such a statement as this must be so completely qualified as to be regarded as altogether inaccurate. No form of cancer is yet known ever to have been cured, either by operation or anything else. If removed it invariably returns, and in all these cases of cancer of the stomach quoted by Mr. Gamgee, save one, the disease speedily returned and killed the patients. The one exception has not yet been under trial long enough to enable us to give an opinion. Doubtless it will have the same end as the others.

VIII.—Function of Periosteum.

The history of the development of our knowledge of the formation and growth of bone is extremely interesting, because it shows how completely misleading are the conclusions based upon vivisectional experiments, and how perfectly the secrets of Nature may be unravelled by a careful and intelligent examination of her own experiments. No one can look now at a necrosed bone without seeing how completely the whole story is there written. The history also exemplifies the fact that it is not only the purely practical details of surgery which are independent of vivisection for their development, but what are called the more scientific developments of physiological knowledge are equally possible without its aid, and are often retarded by its misguidance.

The first real observer in this department was Jean Guichard Duverney, born in 1648, who achieved such distinction that Peyer, in a dedicatory epistle, says to him, "Sempiterna te (Duverneyum) quondam trophœa manebunt et Regi vestro, Academiæ Urbique gloriosum erit tantum aluisse civem." He studied closely, and wrote a great deal about the anatomy, physiology, and surgery of bones, and in his books* he fully describes the method of growth and ossification of

* Traite des Maladies des Os, 1751, Paris. Œuvres Anatomiques, Paris, 1761.

bone, its dependence for its nutrition and growth upon the periosteum; the only thing he lacks is the microscopical knowledge of modern times. He also performed vivisections, not upon the periosteum, but on the medulla, and they led him into most erroneous conclusions. He cut through the thigh bone of a living animal, and repeatedly plunged a stilette into the medulla, and the animal gave evidence of great suffering. The marrow, he therefore concluded, received a great number of nerves, which passed through the canals in the bone, but which existed only in his imagination. As long as he kept to his clinical observations and anatomical dissections he reached exact conclusions, but as soon as he entered the arena of vivisection he went all astray.

The next author of note was Francois Hunauld, born in 1701, who published in 1730 "Recherches Anatomique sur les Os du crâne de l'homme," in which he describes with the utmost accuracy the ossification by the membranes, between which the cranial bones are developed. The only errors he made were hypothetical descriptions of things he could not have seen without a microscope, and that he evidently had not used.

Next comes Robert Nesbit, a Scotch surgeon, settled in London, who published in 1736 an essay, entitled "Human Osteogeny, explained in two lectures."

He was the first to demonstrate the construction of bone by the now familiar experiment of dissolving out the mineral matter, and leaving, as he most accurately says, a spongy substance altogether different from cartilage. Cartilage he referred to its proper function; but he describes it as vascular, in this showing the want of microscopical investigation; but concerning the process of ossification he had got quite as far as we have at the present day. He tells us that in the blood, or in a liquid separated from it, there is an ossifying fluid, a fluid containing the material out of which bone is built up, composed of parts which are not sensible: that whenever Nature determines upon an ossification within a membrane, from which all bones are developed, or in a cartilage, she directs by some means, the

nature of which we are ignorant of, a larger quantity of blood to the vessels of the membranes, so that they become distended and visible, whereas before they were invisible. He describes the process of ossification only with such errors as are due to the absence of the microscope, and says: "Thus the membranes (periosteum) and the cartilages, are the reservoirs in which the osseous particles are deposited and moulded." He denied the existence (and quite correctly) of an internal periosteum which had become about that time a matter of great contention.

The celebrated discovery of the property of madder for staining growing bone, when used as food by animals, was published by John Belchier in the Philosophical Transactions for 1736, and he fully disclosed thereby the method of growth of bone from periosteum, and many other most interesting and valuable discoveries concerning bone.

Between 1739 and 1743 Henri Louis Duhamel-Dumonceau published eight memoirs on the growth and repair of bones, largely based on the suggestive discovery of Belchier. Up to this time the formation of callus was thought to be due to an effusion of osseous juice—a belief which pervaded the surgical teaching of a distinguished professor of the University of Edinburgh so late as my own student days—but Duhamel proved its real origin. He also completely established the fact that bones grow in thickness by the addition of osseous layers originating from the periosteum.

Duhamel performed many vivisections, but it is quite clear from his own descriptions that they were failures and did not help him. He says himself that his conclusions were based on sections which he made of specimens of fractures which were in the collections of Winslow, Moraud, and Hunauld. In fact, to any intelligent observer who looks at a preparation of necrosis it is evident that no vivisection was needed to show the whole process and growth of repairs of bone; and even if vivisection were necessary, history displays with certainty that Syme and Ollier, to whom Mr. Gamgee attributes the merit of these discoveries, were only uselessly repeating the attempts of

Duhamel more than a century old, and were only attempting to establish what had long before been proved.

Since Duhamel's time thousands upon thousands of experiments upon animals are on record, some to prove that the periosteum has nothing whatever to do with the formation of bone or with the production of callus, and others to prove that we owe everything to the periosteum, and yet it has been settled absolutely only by the experiments of disease upon our own bodies, and not by experiments on animals. It would be really amusing to read the accounts of the researches of Sue, Bordenave, Delius, Dethleef, Fongeroux, Haller, and countless others, were not the humour of their mutual contradictions sadly marred by the accounts of the tortures they inflicted uselessly on myriads of animals.

The experiments of Dethleef of Göttingen in 1752 were far more scientific than those of Mr. Syme in 1837, and the conclusions of both seem to me to be equally erroneous. At any rate Mr. Syme did not help us one bit in advance of Duhamel and Fongeroux. Haller made numerous vivisectional experiments, and he was the most distinguished physiologist of his time, yet he records his conclusion that the periosteum has nothing whatever to do with the formation of bone, and as a proof of this he quotes the formation of exostoses on teeth. The fact is, that as long as dependence was placed on vivisection, so long did one experimenter investigate after another fruitlessly, and with conclusions absolutely contradictory. On pathological research alone has the true conclusion been established. Haller made a long series of vivisectional experiments, published in two memoirs,* and triumphantly proved that the periosteum can have nothing to do with the formation of bone. He concluded from his vast array of experiments that bone grew from the middle and not from the outside, together with many other absurdities, only to be matched in the modern researches of Bennett and Rutherford on the function of the liver, also based on fallacious vivisections.

* "Sur la Formation des Os." Lausanne, 1758.

The whole of the physiology and pathology of bone have been laid bare by the accident of the pigs of the dyer with whom Belchier dined, by microscopic research, and the observations of disease. Yet Hunter and Stanley thought it necessary to confirm the conclusions of the madder stain by such a clumsy device as fixing a ring of metal round the growing bones of a young animal, letting the ring remain for months or years, and then examining to find—what? absolutely nothing, save that the ring had been more or less covered, just as it would have been on a tree, thus only repeating Duhamel's conclusions. Other observers bored holes in bones and filled them with metal plugs and shot to find only that the conclusions of disease, that long bones grow from the epiphyses is absolutely correct. Then we come to Mr. Syme's paper in 1837, "On the power of the periosteum to produce new bone." Mr. Syme almost every week was in the habit of cutting through great thicknesses of new bone attached to and growing from the periosteum to get at dead old bone from which the periosteum had been separated; and the new bone, being between the periosteum and the old bone, must of necessity have grown from the periosteum: there was nothing else it could grow from. Therefore, if Mr. Syme found it necessary to cut up animals to find out what was constantly staring him in the face, he was a profoundly unscientific surgeon, whose researches were as badly conducted as they were useless.

When Mr. Gamgee read his paper at the local Medical Society and quoted these experiments of Mr. Syme, I said that, as far as I could recollect, the fact was that their conclusions had been absolutely upset by Mr. Goodsir, who did not make experiments upon animals, but followed a far more scientific method of research—microscopic examination. On refreshing my memory I find this is the case. In a paper read before the Royal Society of Edinburgh * in answer to Mr. Syme, Mr. Goodsir shows that Mr. Syme's method of research was so bad that the experiments could not be performed accurately. Mr. Syme was

* Trans. Roy. Soc. Edin., vol. xiv.

pre-eminently an unscientific surgeon, for he knew nothing of the microscope; in fact, it may be doubted if he ever looked through one. Mr. Goodsir, on the contrary, may be looked upon as the father of modern histological research. He proves conclusively that Mr. Syme's experiments were absurd in their conception and futile in their application. Mr. Goodsir's conclusions are, on the contrary, uniformly accepted, and as to his method he says that they were made upon shafts of human bones which had died,—museum specimens, just as Duhamel's were. They showed that whilst the periosteum is the matrix and machine by which the new bone is made, the real agency is in the layer of osteal cells, and so he finally solved the riddle. He did this by microscopic and pathological research. He condemned the employment of vivisection as useless and misleading, and to him we owe the completion of Belchier's and Duhamel's research,—a completion which was hindered for a century by the blunders of vivisectionists.

After this I need not stop to discuss the useless repetition of Mr. Syme's experiments, with variations by Ollier of Lyons, for that would be merely a waste of time.

IX.—The Ecraseur.

Mr. Gamgee quotes the introduction of the ecraseur as an instance of the influence of vivisection on the progress of human surgery. No more unfortunate instance could be quoted. The principle of the instrument is that it crushes and tears the tissues instead of cutting them as by the knife. The surgical aphorism that "torn arteries don't bleed" was in existence long before M. Chassaignac was born, and if he had based his employment on that alone he could have done all that his instrument has effected. But unfortunately he performed experiments upon animals, and immediately he was led astray. I once saw the leg of a favourite dog amputated at the hip-joint on account of disease, and when the limb was removed not a single vessel bled, and the main artery was tied only as a

matter of precaution. In the human subject I have seen twelve or fifteen arteries tied in the same operation, for with us the smallest arteries bleed and require to be secured. Our arteries act in ways altogether different from those seen in the lower animals. Their pathology and physiology are absolutely different, as may be seen in the frequency of apoplexy and aneurism with us, and the almost complete immunity from them of all the lower animals, even in extreme old age. Hunter tried his best to induce aneurism to the lower animals, and failed. Injuries to arteries in the lower animals are repaired with the utmost certainty and readiness, but in man it is altogether different. It may be easily imagined, therefore, that M. Chassaignac's application of the ecraseur to the lower animals was found wholly misleading when man was the subject, and now in human surgery its utility is extremely limited; that is, it is entirely confined to operations where only very small arteries are divided. Speaking for my own practice, I may say that it might be dispensed with and never missed.

Mr. Gamgee's quotation of its application to the ovarian arteries of the cow is peculiarly unfortunate, seeing that when it was used for the same purpose in the human subject it had speedily to be given up on account of its failure.

X.—Detection of Poison.

A great deal has been made of the successful experiments recently performed by the medical experts for the conviction of Lamson, for that worst of all crimes, the most unpardonable, murder by poisoning. At first sight this does seem a case in which experiments upon animals may be justified. Certainly anything and everything ought to be done to convict a poisoner, and if nothing short of that would do, I would advocate the performance of a hecatomb rather than that such a scoundrel as Lamson should escape. So late as a few weeks ago I made a reservation on this point in my condemnation of vivisection as a method of research, but it seems to me, from a closer con-

sideration of the facts of the case, that it forms really a very strong argument for the complete abolition of vivisection, and, at the same time, unfortunately it is a matter of grave reproach to modern science.

Fortunately the conviction of a poisoner is almost certain. If he is not a doctor he commits the crime so clumsily that he cannot escape. If he is a doctor he must have an interest in the victim's death, is almost certain to be in pecuniary difficulties, and is sure to have had a bad character previous to his great crime. The only difficulty lies in the proof of the presence of the poison. With all poisons but the alkaloids this is a matter of such ease that failure is impossible, and as the alkaloids are almost exclusively in the hands of chemists and doctors the limitation of their use is very close.

The most notorious case in which an alkaloid was used, or supposed to have been used by a poisoner, was that of Parsons Cook. The alkaloid was supposed to be strychnine, and I say supposed, because I rise from the perusal of that trial with much doubt as to whether Parsons Cook really died of strychnine poisoning. Certainly I cannot accept it as proved, and I think if the trial were to occur now the same evidence which convicted Palmer would probably break down. I am perfectly satisfied, however, that Palmer received substantial justice.

In Palmer's case the principal witnesses for the prosecution were the late Dr. Alfred Swayne Taylor, and the late Sir Robert Christison, certainly the greatest toxicologists of this century. Strychnine was not discovered in the body of Cook, and Dr. Taylor had to admit that the best tests then known were insufficient to discover one fiftieth of a grain, and that even half a grain might remain undetected amongst food in the stomach. Palmer was sentenced to death upon the 27th of May, 1856, and in July of the same year a method of chemical analysis was published by Copney in the "Pharmaceutical Journal," by which one five hundred thousandth of a grain of strychnine could be detected with certainty after separation. In his evidence Dr. Taylor admitted that the experiments he had performed upon

animals with strychnine were practically worthless for any application to man, and in the report of the Royal Commission of 1876 he condemned such experiments, particularly those which are directed towards the discovery of an antidote to snake bite.

Strychnine was discovered in 1818, and was first used as a poison in 1831, and again in the case of Mrs. Sergison Smith in 1847, and it was no new matter the toxicologists had to do with in the trial of Palmer. It must be regarded, therefore, as a matter for deep regret that it was not till after the trial and execution of Palmer that the chemistry of strychnine was exhaustively examined, and definite and certain tests for it obtained. At the trial there was a sort of competition among the vivisectionists, and Serjeant Shee actually urged as an argument for the defence that his witnesses had performed ten times more experiments to prove that there was no strychnine, than the witnesses for the prosecution had performed to prove what never was proved, that strychnine was used at all. Yet in two months chemical processes were devised without the slightest aid from vivisection, which detected half-a-millionth of a grain with certainty.

At the trial Professor Christison said that another alkaloid was known, of a deadly poisonous character, which it was impossible to detect, but under the judge's direction he refused to make its name known. There were really many alkaloids of a deadly poisonous character at that time quite well known, and aconitine was one. The first case to bring this poison under notice as a criminal agent was in 1841, and the notorious Pritchard destroyed his victims with it in 1865. Dr. Penny of Glasgow resorted to experiments on animals in order to bring the crime home to Pritchard, and succeeded. Yet I have looked in vain for any record of a research for a method which will detect aconitine with certainty by chemical analysis, as strychnine can be detected, and Dr. Stephenson admitted in evidence that there was no such test.

I daresay such a method will be shortly published, and what I desire to point out is that this discovery ought to have

been made long ago in the interest of public safety, not only with regard to aconitine, but with regard to many other alkaloids which may be used in the same way, and which cannot be discriminated from aconitine, even by experiments on animals. At present, when need arises, we must go back to the uncertain method of experimenting upon animals. But this is not science, if by that word we are to speak of exact knowledge. The very weakness of this method has led to a serious infraction of the principles of our judicial proceedings, for the Home Secretary announced in the House of Commons only a few nights ago that the Government, in a case such as Lamson's, could not allow the proceedings of the medical experts for the prosecution to be watched by other experts on behalf of the defence.

This is altogether unfair, for with such an uncertain and inconclusive method as that of experimentation on animals two men, even if appointed by the Colleges of Physicians and Surgeons, and not by the Treasury, may be mistaken, whereas by chemical or spectroscopic analysis mistakes are extremely unlikely, and the more observers there are the better.

The general conclusion therefore is, that for such purposes experiments on animals should be entirely prohibited, and that an exhaustive research should at once be undertaken at the expense of the State, upon the spectrum and chemical analysis of all substances which may be used for criminal purposes There is no known substance of constant character which has resisted the chemists' effort to identify it when it has been properly investigated.

If all these alkaloids had been subjected to an exhaustive investigation as strychnine was after Palmer's trial, there would have been no need to revert to vivisection in order to convict Lamson, and I do not think it would now be contended as necessary for the detection of a poisonous dose of strychnine that experiments on animals should be made. Vivisection in this case is therefore not the weapon of science, but is the refuge of incomplete work.

I have now gone over all the points urged in favour of

vivisection as contributory to surgical advance as given in Mr. Gamgee's pamphlet, and with the result, to my mind, of proving that in every instance the claim is groundless. Had I time at my disposal I could examine in detail numerous other claims equally fallacious. So far, indeed, as I have already said, I have not met with a single case capable of substantiation, not even the most recent—that of Pasteur's discovery of the prevention of zymotic diseases in domesticated animals by inoculation of cultivated virus.

In the *Nineteenth Century* for March will be found an article by a well-known veterinary surgeon, Mr Fleming, on this subject. He describes the ravages of such diseases as anthrax, splenic fever, rinderpest, swine plague, etc., amongst the animals which form our food supply, and I admit the accuracy of his statements. Quite recently Mr. Pasteur has discovered, and his statements have been amply confirmed, that the specific organisms which form the poisons of these diseases, may be so artificially cultivated as to be capable of producing by inoculation a mild form of the original disease, which mild form is largely protective from the severe and fatal form of the same malady. In fact there is a perfect analogy between this discovery of Pasteur and that of Jenner.

The argument is that by their inoculation the zymotics of domestic animals may be stamped out, and the claim is that it is a great advance brought about by vivisection. But on a little examination it seems to me that both argument and claim break completely down. If it is really an advance from vivisection, then those who benefit are the animals experimented upon, and that may be legitimate enough—they at least would share largely in the benefit.

But the case must be examined from another side. There are some twenty zymotics amongst our domestic animals to be provided against. Are we to have each of them inoculated some ten or twelve different times, each time for a different disease? The affirmative reply possesses a strong pecuniary interest for a veterinary surgeon, but a practical man will only smile at it.

But, to go deeper into the question, we find another and a much stronger objection. Such a process as protective inoculation must always be an inefficient and a temporary measure. To take the case of vaccination and small-pox, it is beyond dispute that vaccination protects the individual to a large extent from small-pox, but it does not protect the community—as may be seen from the ravages it is making at the present time in neighbouring towns and counties. The machinery of vaccination never can be so perfect as to stamp out the disease, and it must be regarded purely as a temporary expedient. The real agent for the stamping out of small-pox is the machinery of a system of sanitary police, such as we have here; and even on the small scale in which we have had it for six years it has worked marvels. It will stamp out not only small-pox but every other zymotic at the same time, and by the same measures, and then we need not trouble about vaccination—certainly it need not be compulsory.

But the case is still stronger with the lower animals. With them, as with us, civilisation has introduced zymotic poisons, which are absolutely unknown to the wild animal, and the reasons are not far to seek. In my capacity as one of the managers of a large public institution, I had recently to investigate the cause of an endemic of swine plague, and I found a state of matters which had caused at the same time typhoid fever in a human patient.

Look at the arrangements of an ordinary British farm yard, and then believe that it is a matter of no wonder that rinderpest destroys the cattle, and diphtheria the farmer's children. The animals spend their lives in houses not lighted and not ventilated, or walk about in a mass of seething filth, on one side of which stands the farm house, every room reeking with the stench of the cattle yard.

When it begins to dawn on the mind of the British public that all these diseases, both for man and animals, are absolutely preventible by the simple means of securing fresh air, pure water, and abundant light, they will be banished. Meantime

inoculation may, and probably will, prevent individuals being attacked, but it will not stamp out the diseases, and it must be regarded as really a retrograde proposal when we have in our hands the means of complete prevention.

I hope I have thus made it clear that deeply as I feel the strength of the objection to the practice of vivisection upon the various grounds I indicated at the beginning of my paper, I urge against it a far stronger argument than these, that it has proved useless and misleading, that in the interests of true science its employment should be stopped, so that the energy and skill of scientific investigators should be directed into better and safer channels. I hail with satisfaction the rousing which is evident in the public mind upon this question, and I feel confident that before long the alteration of opinion which I have had to confess in my own case will spread widely amongst the members of my useful profession.

HERALD
BIRMINGHAM
PRESS
FORWARD